pute it with some approach to accuracy, by more than one method. While the breadth of the crust-waves must vary with the energy of the earthquake, it can be shown to have been, in some of the more violent of these convulsions, enormously great; in the earthquake of Conception probably ten or eleven miles, and in that of Lisbon as much as twenty-five miles.

Professor Rogers concluded by stating, that the lateness of the hour induced him to withhold the sequel of his paper, the design of which was to apply the generalizations which he had presented, to the explanation of the origin of those great flexures of the strata so magnificently displayed in the mountain chain of the United States; and he announced his purpose to resume the subject at some more appropriate season.

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## SPECIAL MEETING.

*Third Session, 27th May,* 10 *o'clock, A. M.*

Dr. Patterson, Vice-President, in the Chair.

The Secretaries presented a letter addressed to them by Mr. Sears C. Walker, and Professor E. Otis Kendall, of the High School, "On the Great Comet of 1843." This letter is dated High School Observatory, May 27, 1843, and, omitting a few paragraphs, is as follows:—

Gentlemen,—We avail ourselves of the centennial meeting of the members of this Society to lay before them, generally, the reasons which induce us to believe that the recent visiter is a comet of short period, only $21\frac{7}{8}$ years, and that it is identical with those of February, 1668, and of December, 1689. An early suggestion of its identity with that of 1668 was made, we believe, by Prof. Peirce, in a lecture delivered at Boston on the 23d of March last. Shortly before that date, viz. March 20, it appears to have been noticed by Mr. Cooper, of Nice, in a letter to Schumacher, published in the London Times. The question of their identity has been discussed by Prof. Schumacher and Mr. Petersen, of Altona. The latter applies Galle's elements to the perihelion passage in 1668, and Prof. Schumacher expresses an opinion in favour of their identity. The subject has been more fully discussed by Mr. Henderson, the Astronomer Royal of Scotland, who, in a letter to Schumacher of April 11th, states that "there appears great probability in favour of the supposition that the late comet, and the one which appeared in 1668, are the same." Mr. Henderson then gives the elements of the comet of 1668, and a comparison of the ephemeris computed from them, with the places of the nucleus of the comet, as found by Mr. H. on a map in his possession, containing a trace of its path among the stars from March 9th to March 21st, 1668, as seen at Goa. The agreement is quite sufficient to warrant a conclusion of their identity. The first suggestion of the identity of the comets of 1689 and 1843, was made by ourselves in a letter to the editor of the Philadelphia Gazette, April 6th, in which, after giving our own elements of this comet, and Pingré's elements of that of 1689, we mentioned "these elements agree quite well with Prof. Peirce's and ours, except the inclination. The observations used by Pingré are pronounced to be good by Olbers, and he expresses confidence in the elements of Pingré. Still the imperfections of instruments and catalogues of stars in 1689, may have caused such imperfections of the observations as to lead Pingré to an inclination of 69°, instead of 39° or 36° as found at present. When we consider that the inclination found by Prof. Peirce and ourselves is derived from an orbital motion of less than 2°, it is manifest that the position of the plane of the orbit, or, in other words, the inclination, must be quite uncertain. The same difficulty must have occurred in 1689, under still more unfavourable circumstances. It is quite likely, therefore, that a modification of the elements of this comet, not greater than those of Halley's comet in its successive periods, would represent the observations used by Pingré, as well as his own elements, or at least within such limits as those to which the errors were liable."

In a communication in the Inquirer of the 11th April, we still repeated our suggestion of the sameness of these comets. Finally, in the Boston Courier of April 25th, Prof. Peirce published his elements of the comet of 1689, and found an inclination smaller even than that of 1843, with other elements agreeing very well with those of the recent comet. This removed all doubt in our minds of the identity of these comets, and on the arrival of the London Times of April 14th, containing Schumacher's opinion confirmatory of Prof. Peirce's of the sameness of the comets of 1668 and 1843, we compared the periods, to see if the comet of 1843 could not be both that of 1668 and 1689; and we found that a period of $21\frac{7}{8}$ years would answer for all three. We announced this conclusion in a letter dated May

8th, in the United States Gazette of May 11th, with an attempt to account for its not being seen except about the eighth period of its revolutions, when it returns to the perihelion at the same season of the year. We also stated that our parabolic elements, which gave an orbit passing through our first and last normal places of March 20th and April 9th, gave the place on the middle date of March 30th too much advanced. We also stated that such was the case of all the good parabolas obtained for its orbit in Europe or America, and mentioned our coincidence in opinion with Encke, that the parabola was not the true orbit, and added, that probably it would be found to be an ellipse of $21\frac{7}{8}$ years. We also stated, that an attempt further to correct the parabola for the middle observation, would lead to a paradox such as Encke had encountered in his attempt to complete an orbit, on the presumption that the curve is a parabola. We immediately, with the kind assistance of Mr. John Downes, commenced the computation of an orbit on Gauss's general method, without presuming upon any conic section, but hoping to find an ellipse, and found a double paradox, a comet moving in an hyperbola, and that hyperbola having its perihelion point within the body of the sun. We immediately announced this result in the United States Gazette of the 19th April, and invited an expression of opinion from astronomers as to the legitimate interpretation of this result. It was manifest, that if the centre of gravity of the comet and tail was moving away in a non-periodical curve, our favourite opinion of the identity of these three comets, and short period of $21\frac{7}{8}$ years, would be untenable. Although we considered the hyperbolic orbit as well as the small perihelion distance to be both paradoxical, we were willing to submit them as genuine deductions from our observations and computations, and leave them to be received as paradoxes, or explained away, as the sequel should show. In so doing, we postponed, for the time, urging our favourite theory of the short period of $21\frac{7}{8}$ years. It is true that we had suggested the probable cause of the acceleration of the comet's place for the middle observation, as computed from a parabolic ephemeris, to be owing to the shape of the comet, in the United States Gazette of the 6th of April, after pointing out the acceleration of the comet's place for the middle observations, viz.—"The slight difference between the two curves (our parabola and the true path of the comet) is lost amidst the errors of observation, and the uncertainty whether the central portion or the densest part of the nebulosity corresponds with the actual centre of gravity." We were aware that Encke had resorted to this hypothesis to explain the paradox of the acceleration of his comet, previous to his more fortunate suggestion of the resisting medium. In regard to the recent comet, our attention was early called to this source of error by our esteemed correspondent, Mr. E. C. Herrick, of New Haven, who, in a letter addressed to S. C. Walker, on the 29th of March, remarks, "The concentration of light in the nucleus (as seen in the ten feet Clark telescope of five inches aperture) seemed to me, on two occasions, to be considerably nearer the anterior than the posterior part. Once we thought we could detect three dim star-like points, but it was almost impossible to decide with certainty. *Where the tail is so immense, is there not some hazard in assuming the centre of the nucleus to be the centre of gravity of the whole body?*" We are particular about the dates of these suggestions respecting the centre of gravity of the comet and tail, inasmuch as it is found to be a matter of much importance in the sequel. Having fairly, on the 19th and 20th, laid our two paradoxes, viz. the hyperbolic orbit, and the perihelion distance less than the sun's semidiameter, before the public, with some suggestions as to the inferences that would follow from a strict interpretation of this result of calculation and observations, viz. that of the necessity of a rebound, or of the comet's flowing round the sun, we waited for the opinions of our friends, and for further information from the European observatories. We have since received both, and hasten to lay them before you. First, the arrival of the Caledonia brought out the announcements from most of the European observatories in Prof. Schumacher's excellent Astronomical Notices of April 22. From these it appears that the comet's nucleus was first seen in Europe, at Nice, on the 14th, and first observed at Rome on the 17th of March. This was five days later than it was observed at several places in the United States, viz. on the 9th and 11th, not to mention Mr. Clark's measures of the distance of the nucleus from the sun on the 28th of February. The latest observation quoted by Schumacher, is that of Encke at Berlin, March 31st. Perhaps it was seen later. We followed it at the High School Observatory till the 10th of April. The conclusion of Encke, Steinheil, Nicolai, Schumacher, Argelander, and others, that the parabola is not the true conic section for this comet, confirmed the announcement we had made on the 11th April. Encke, who alone of all the astronomers yet heard from, had discussed the question of the particular conic section, had found an hyperbola resembling ours, with the perihelion point just falling outside of the sun. Thus one of our paradoxes, that of the hyperbolic orbit of the observed centre of the nebulosity, was confirmed by the only astronomer in Europe, who, as far as heard from, had gone over the same ground with ourselves.

For the other paradox, viz. a perihelion point within the body of the sun, we find the most ample confirmation. This element is thus stated by the European astronomers:—

| | |
|---|---|
| Plantamour, Geneva, | 0.0045 |
| Arago, Paris, | 0.0054 |
| Galle, Berlin, | 0.0118 |
| Argelander, Bonn, | 0.0072 |
| Nicolai, Manheim, | 0.0037 |
| Encke, Berlin, | 0.0047 or less. |
| Do. do. | 0.0036 |
| Do. do. | 0.0052 hyperbola. |
| Mean, | 0.0057 |
| Do. omitting Galle, | 0.0049 |
| Our last result, | 0.0041 hyperbola. |
| Sun's semidiameter | 0.0047 |

Thus it appears that Plantamour, Nicolai, and Encke, on two occasions, had encountered the same paradox as ourselves, viz. that of a perihelion point within the sun. It is also remarkable that none of the orbits, except Encke's hyperbola, suffice to represent the observed path of the centre of the nebulosity among the stars.

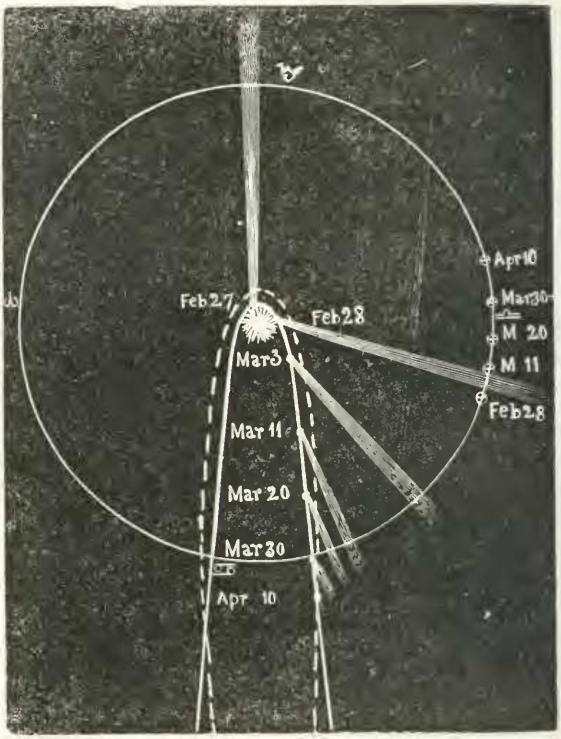
Hence it appears, from the concurrence of authorities on these subjects, that good observations of the path of the centre of the nebulosity, carefully reduced, lead to a hyperbolic orbit, and an approach of centres of the sun and comet as near as their physical qualities will permit.

In this stage of the inquiry, the principal difficulty consists in reconciling these two paradoxes with our favourite opinion of the identity of the three comets of 1668, 1689, and this year, with a short period of 21⅔ years. Now it is fortunate, that in the case of our hyperbola the same natural and plausible explanation that does away with the one paradox does away with the other. The true key to the solution of the difficulty is, we are persuaded, the suggestion first made to us by Mr. Herrick, March 28th, and first suggested to the public, by ourselves, in the United States Gazette of April 6th, viz. the "uncertainty whether the central or densest portion of the nebulosity corresponds with the actual centre of gravity." We now proceed to state the opinions of our esteemed friends and correspondents on this point. Dr. Anderson, of New York, writes under date of May 19th and 22d, stating, unhesitatingly, that the analogies in favour of the identity of the comets of 1668 and 1689, should lead us to reject the hyperbolic orbit as being unnatural in itself, and wholly irreconcilable with these analogies; and that we should rather regard this hyperbolic orbit, and too close perihelion distance, as the consequence of some error in the data, or in the methods, or in the computations. That there is nothing in the effect of contact of the bodies, or resistance of the comet by the atmosphere of the sun, which could change the character of the conic section from one of a less velocity to one of a greater. From Professor Alexander, of Princeton College, we have received a letter dated May 20th, in which he proposes an explanation of the difficulty, at once simple and natural, and fulfilling all that was required by Dr. Anderson. It is based on the supposed occurrence of the very error against which we were cautioned by Mr. Herrick, March 28th, and which we alluded to in our published letter of April 6th, namely, the error arising from measuring the place of the densest point of the nucleus, instead of the common centre of gravity of the nucleus and tail. We give below his letter in full. We have also had placed in our hands, by Professor A. D. Bache, a letter from Professor Bartlett, of West Point, dated May 23d. We give below that part of his letter which treats on this subject, remarking, that we have no doubt that the coincidence in opinions of Mr. Herrick, Professor Alexander, and Professor Bartlett, has taken place without either one having any knowledge that the same idea had occurred to the other two. We would also remark, that the criticism of Professor Bartlett on Arago's parabolic elements and on our own, is just, and confirms our opinion, that no parabolic ephemeris will perfectly represent a series of observations of this comet. We know of only two sets of elements that will give a good ephemeris; the one is Encke's, and the other is ours. Both are hyperbolic and paradoxical. We give them below. The explanation of Professors Alexander and Bartlett, we have no doubt, is the true one. It is plain and natural, and, *a priori*, extremely probable. It will also satisfy the criticism of Professor Anderson, inasmuch as it points out the particular source of the error of the data, which Dr. Anderson supposed must exist somewhere. The explanation is doubly satisfactory for ourselves, since it leaves the way clear for the establishment of the short period, and the identity of the three comets of 1668, 1689, and 1843, and leaves us still a hope of seeing this remarkable visiter in 1865. Moreover, it does away with both paradoxes, and shows, at the same time, that the European astronomers, as well as ourselves, who were led into them, arrived at them in the legitimate and only possible mode of observation and computation.

Professor Bessel, of Königsberg, the greatest living astronomer, and, since Olbers's death, the most experienced and sagacious observer of comets, remarks, in a letter to Prof. Schumacher, dated March 28th,—"This comet seems to have expended the greater part of its nucleus in building up its splendid tail."

We are happy to add the testimony of our friend Mr. Nicollet, in favour of the strength of these analogies, and of the probable return of this comet in 1865, as an inference not to be in the slightest degree shaken by the fact, that a nice discussion of the observations of the apparent centre of the nebulosity has led to the two paradoxes already quoted. We hail the favourable opinion of this distinguished traveller, who received the Lalande medal for the discovery and the elements of the comet of the year 1821. We are happy further to add the testimony in favour of the plausibility of the period of $21\frac{7}{8}$ years, communicated to us in writing, or verbally, of our valued friends, Alexander, of Princeton; Mitchell, of Nantucket; Gilliss, of Washington; Herrick, of New Haven; Loomis, of Western Reserve; and, nearer home, of Professors Patterson and Bache.

A letter from Prof. S. Alexander to Mr. Walker, dated Princeton, 20th May, 1843, after remarking that the supposition of the comet having actually struck the sun or his envelope, and then *rebounded*, is too violent to be admitted, except in the absence of all other rational explanation, proceeds to suggest the following as perhaps a plausible solution of the difficulty. "The centre of gravity of the comet of 1843," he says, "was at an unusual distance from that which seemed to be the actual nucleus: this led to an erroneous estimate of the comet's position. As, moreover, the comet, when first observed, was nearly in its perigee, it is altogether possible that the error arising from the cause here suggested, was, at the same time, at its maximum, and that it continually decreased until the comet disappeared. The effect upon the relative position of the apparent and true orbits would consequently be such as is roughly represented in the following diagram—the true or dotted orbit deviating more and more from the apparent, as we retrace it in the direction opposite to the comet's motion, and thus escaping the sun at the perihelion."




Professor W. H. C. Bartlett, of West Point, writes to Professor Bache, under date of 23d May, 1842, as follows:—

"The more immediate purpose of this letter is to suggest to yourself and the Society, what has appeared to me a possible explanation of the very great discrepancies between the observations and both the ephemerides computed from M. Arago's and Mr. Walker's elements.

"I suppose that the *apparent* orbit of the comet is different from the *true;* or that the path of the nucleus is not the same as that described by the centre of gravity of the entire mass. To illustrate my meaning, suppose the comet to approach the sun in a parabolic or very elongated elliptical orbit, which will be, by the principles of physical astronomy, the path of the centre of gravity. As the comet approaches the perihelion, let it be greatly but gradually elongated in the direction of a line joining the nucleus and the sun, the tail being thrown off in a direction from this latter body; and suppose this to result from the repulsive action of the cometary particles upon each other, in consequence of the heating influence of the sun, in the same manner as the elastic force of vapour is increased by an elevation of temperature. The action being limited to the particles upon each other, the centre of gravity will be undisturbed, and continue to describe its regular orbit, from which each extremity of the elongation will recede on the line of the radius vector, though in unequal degrees, till it reaches a maximum, resulting from an equilibrium between the elastic force of the cometary medium and the weight of its elementary particles, or the force by which they are drawn towards the centre of the mass.

"The expansive action here supposed, would, in the nature of things, be gradual; and hence, before the nucleus, or the *thing observed*, could be totally resolved into a vapour like the tail, and thus disappear, the reverse action would begin, in consequence of the rapid retrocession of the comet from the sun. The disturbed motion of the nucleus being, for a part of the time, *from* the true orbit, or that of the centre of gravity, *towards* the sun, the observations, if made at this time, would give a constantly increasing eccentricity, or diminishing perihelion distance; and thus the perihelion itself might be brought apparently within the surface of the sun, while not a particle of the comet's matter would touch that body. The observations, if made while the mass of the comet is contracting towards its centre of gravity, would give an increasing perihelion distance till this point is again brought within the nucleus in the depths of space."

We subjoin Encke's hyperbolic elements, and also our own. The latter have been recomputed, after correcting a slight oversight in our calculations, kindly pointed out by Prof. Anderson. Encke's ephemeris agrees closely with his observations. Our hyperbolic elements give an ephemeris corresponding with our normal places within one second of space.

| | *Encke.* | *W., K. and D.* |
|---|---|---|
| Perihelion passage, | Feb. 27$^d$.49778 m. t. Berlin. | 27$^d$.58939 m. t. Green. |
| Longitude of perihelion, | 279° 2′ 29.9 m. eq. March 0 | 280° 44′ 3.7 m. eq. March 30 |
| Long. asc. node, - - | 4 15 24.9 | 15 57 3.2 |
| Inclination, - - - | 35 12 38.2 | 34 19 52.0 |
| Eccentricity, - - - | 1.00021825 | 1.00090495 |

| | | |
|---|---|---|
| Gaussian angle,* - - | 1° 11′ 49″.0 | 2° 26′ 12″.1 |
| Perihelion distance, - - | 0.00521966 | 0.00410367 |
| Daily motion retrograde, | 13″.175559 | 159″.58936 |

It will appear on comparing these elements, that they agree very well, excepting the eccentricity and its secant, the Gaussian angle. This is always the most uncertain element in such investigations. That there can be no error in the process of computation by Mr. Downes and ourselves, is shown by the fact, that the elements reproduce by computation our normal places, after applying the following small corrections, viz:—

| | | |
|---|---|---|
| March 20.5 | R. A.—0″.6 | Dec.+0″.7 |
| ,, 30.5 | R. A.—0 .0 | Dec.—1 .0 |
| April 9.5 | R. A.—0 .6 | Dec.+0 .3 |

These normal places were obtained from a comparison of all our observations with the best ephemeris we could obtain, which was computed from our elements at our request, by Mr. John Downes, the editor of the United States Almanac, and obtained from the average corrections concurring together near the 20th and 30th of March, and 9th of April, for Greenwich mean midnight. These are more correct than the result of any single measure. We give them for the use of astronomers, freed from refraction, parallax, and aberration.

| | | |
|---|---|---|
| March 20$^d$.5, | R. A. 46° 4′ 38″.4 | Dec. S. 9° 9′ 45″.5 |
| March 30$^d$.5, | 59 51 1 .2 | 6 46 32 .5 |
| April 9$^d$.5, | 68 56 41 .6 | 4 45 35 .7 |

Let us now consider the period belonging to the mean motion. It is obvious, that if we adopt the explanation of Messrs. Alexander and Bartlett, the mean motion and consequent period of the centre of the *nebulosity* observed, and of the real centre of *gravity*, must be the same. This is a necessary condition, since they both arrive at the perihelion point at the same instant of time. Now the earth's sidereal motion in a mean solar day is 3548″.18761. The mean motion of the apparent centre of the nebulosity by our elements is 159″.58936. This, if in an ellipse, would give a period for the apparent centre of the nebulosity, and consequently for the actual centre of gravity, of 22.2339 years.

* This angle is the arc whose secant is the same as the eccentricity.

We now present the argument, *a priori*, derived from analogy. the longitudes being referred to the equinox of the present time.

| No. | Comet. | Lon. of Perihel. | Lon. of Node. | Inclination. | Perihelion Distance. | Perihelion Passage. |
|---|---|---|---|---|---|---|
| 1 | Comet of 1668 | 279°.6 | 359°.8 | 35°.9 | 0.0048 | Feb. 28.8*d* |
| 2 | Comet of 1689 | 273 .5 | 346 .5 | 30 .4 | 0.0103 | Dec. 2.13 |
| 3 | Comet of 1843 | 280 .5 | 348 .5 | 39 .3 | 0.0087 | Feb. 27.54 |
| 4 | ,, | 272 .3 | 356 .5 | 36 .6 | 0.0147 | 27.20 |
| 5 | ,, | 262 .7 | 357 .7 | 36 .7 | 0.0541 | 27.55 |
| 6 | ,, | 277 .0 | 361 .3 | 35 .7 | 0.0082 | 27.45 |
| 7 | ,, | 274 .8 | 359 .1 | 35 .9 | 0.0109 | 27.09 |
| 8 | ,, | 274 .5 | 357 .6 | 36 .4 | 0.0113 | 27.46 |
| 9 | ,, | 279 .2 | 359 .9 | 36 .0 | 0.0045 | 27.46 |
| 10 | ,, | 281 .4 | 365 .9 | 35 .0 | 0.0030 | 27.37 |
| 11 | ,, | 275 .5 | 359 .0 | 36 .1 | 0.0104 | 27.54 |
| 12 | ,, | 277 .5 | 361 .0 | 35 .7 | 0.0071 | 27.47 |
| 13 | ,, | 280 .5 | 364 .6 | 35 .2 | 0.0037 | 27.39 |
| 14 | ,, | 278 .8 | 362 .2 | 35 .5 | 0.0054 | 27.41 |

No. 1. By Henderson, Astronomer Royal of Scotland.
2. By Prof. Peirce, from Pingré's places.
3. By Prof. Peirce, from his and Mr. Bond's places.
4. By Messrs. Nooney and Hadley, from Walker and Kendall's early observations.
5. By Prof. Anderson, from Prof. Bartlett's places.
6. By Prof. Anderson, from later places of W. and K.
7. By Prof. Alexander, from his own places.
8. By Mr. Galle of Berlin, do.
9. By Mr. Plantamour of Geneva, do.
10. By Prof. Encke, do.
11. By Walker, Kendall and Downes, do.
12. By Argelander, do.
13. By Nicolai, do.
14. By Laugier and Mauvais, do.

These arguments seem quite conclusive, and indicate the period of the comet of $21\frac{7}{8}$ years, and its identity with some of the many others, quoted by Pingré in his Cometography, as having occurred in the three series of cycles of 175 years (eight revolutions), which precede the respective dates of its recent appearance in 1843.2, its expected appearance in 1864.9 or 1865.0, and in 1886.9. They also completely confirm the observation made by Messrs. Herrick and Bradley, of the eccentricity of the densest portion of the nebulosity in that nebulosity. They confirm the remark we published in the United States Gazette of April 6th. They confirm the coincident opinions of Professors Alexander and Bartlett. They explain away the seeming paradoxes of the hyperbolic motion of the apparent centre of the nebulosity, and of the tendency of this fictitious curve to a perihelion point within the sun's surface, while the true ellipse of $21\frac{7}{8}$ years' period has a perihelion distance greater than the sun's radius, leaving the comet free to depart and (as we hope) to return about the 1st of January, 1865, to be seen under more favourable circumstances than at this visit.

We conclude by expressing our great satisfaction at the explanation of Profs. Alexander and Bartlett, which, with the computations of the new orbit, by Henderson, for the comet of 1668, and by Prof. Peirce for 1689, have removed the only known obstacle to the admission of the period of $21\frac{7}{8}$ years, and the elliptic orbit suggested by ourselves on the 8th inst.; accordingly we offer it to the members of the Society, on this their centennial celebration as the probable period of this remarkable comet.

If we admit this hypothesis, and suppose that the perihelion distance was possible, that is, for instance, greater than 0.0047, then we shall find the elliptic elements of the comet's orbit the same as the hyperbolic, omitting the Gaussian angle, and making the eccentricity greater than 0.9994.

The actual elliptic elements may be found on this hypothesis, by assuming the above value of 0.9994 for the elliptic eccentricity, and then giving to the difference between the elliptic and hyperbolic radii vectores the form of a constant quantity, multiplied by the reciprocal of the square of the elliptic radius vector. This constant should then be determined from the series of observations by the method of least squares. The elliptic elements should of course be used in computing perturbations.

The following letter from the same gentlemen, which was communicated to the Society at a subsequent meeting, forms an appropriate supplement to the foregoing.

*High School Observatory, Philada. June* 16, 1843.

To the Secretaries of the American Philosophical Society:

Gentlemen,—Since writing the letter which was read at the centennial meeting of the Society, we have compared our normal places of the comet on the 20th and 30th of March with the European observations. We have not been able to find any later than the 31st of March, and must still rely on our own measures for the comet's place on the 9th of April. In order to test the normal places for March 20th and 30th, we subjoin the differences therefrom of the European observations referred to the date of Greenwich mean midnight, after rejecting two in all, whose discrepancies from the mean result exceeded fifty seconds of space.

| Observation compared with | | | Correction of normal place, $\Delta\alpha$. | Correction of normal place, $\Delta\delta$. | Date of normal place. |
|---|---|---|---|---|---|
| Paris, | March | 19.5 | + 7″.2 | + 2″.2 | March 20.5 |
| Geneva, | ,, | 19.5 | + 7 .0 | + 2 .1 | |
| Rome, | ,, | 19.5 | +31 .0 | + 5 .0 | |
| Rome, | ,, | 20.5 | — 7 .3 | +35 .1 | |
| Berlin, | ,, | 20.5 | + 5 .4 | —23 .9 | |
| Munich, | ,, | 20.5 | + 3 .4 | —21 .4 | |
| Manheim, | ,, | 21.5 | —13 .9 | —26 .1 | |
| Geneva, | ,, | 21.5 | — 2 .6 | —28 .3 | |
| Bonn, | ,, | 21.5 | + 0 .5 | —28 .1 | |
| Berlin, | ,, | 21.5 | — 7 .1 | —20 .9 | |
| Munich, | ,, | 21.5 | —23 .5 | + 0 .5 | |
| Vienna, | ,, | 21.5 | | +31 .0 | |
| Mean correction, | | | —00″.0 | — 6″.1 | |
| Manheim, | March | 29.5 | + 0″.9 | —13″.9 | March 30.5 |
| Bonn, | ,, | 29.5 | — 3 .3 | —16 .1 | |
| Manheim, | ,, | 30.5 | +20 .6 | +15 .5 | |
| Berlin, | ,, | 30.5 | + 2 .1 | —19 .5 | |
| Berlin, | ,, | 31.5 | +33 .3 | — 0 .8 | |
| Mean correction, | | | +10″.7 | — 7″.0 | |

If we allow to the High School observations the same weight as that of one European observatory, then the normal places of the point of observation of the comet's nebulosity will stand thus, being freed from parallax and aberration.

| Greenwich mean time. | Normal place, R. A. | Correc. R. A. | Corrected normal place, R.A. | Normal place, Dec. | Correc. Dec. | Corrected normal place, Dec. |
|---|---|---|---|---|---|---|
| Mar. 20d.5 | 46° 4′ 38″.4 | +0″.0 | 46° 4′ 38″.4 | —9° 9′ 45″.5 | —5″.4 | —9 9′ 50″.9 |
| Mar. 30 .5 | 59 51 1 .2 | +9 .5 | 59 51 10 .7 | —6 36 32 .5 | —5 .2 | —6 36 37 .7 |
| April 9 .5 | 68 56 41 .6 | +0 .0 | 68 56 41 .6 | —4 45 35 .7 | —0 .0 | —4 45 35 .7 |

Then the corrections of the ephemeris, computed from our hyperbolic elements, will be

| | | |
|---|---|---|
| March 20.5 | $\Delta\alpha = -0''.6$; | $\Delta\delta = -4''.5$ |
| March 30.5 | ,, $+9''.5$; | ,, $-6''.2$ |
| April 9.5 | ,, $-0''.6$; | ,, $+0''.3$ |

These values are so small that a change in the elements of the orbit of the point of the nebulosity observed, which should reduce them to zero, would be too small to indicate any change in the conclusions already drawn by us from our first normal places. Unless, then, further observations shall be obtained from the southern hemisphere previous to the perihelion passage, we see no way of avoiding the conclusion that the point of the nebulosity observed was moving in an hyperbola, with a mean daily motion of about 160″, which in a curve having a periodical character, would give a duration of a revolution of about 22 years, with elements, as far as we know, identical with those of the comets of 1668.2 and 1689.9.

We subjoin from Pingré's Cometography a list of comets that have appeared at dates when this comet, if it be the same as those of 1668.2, and 1689.9, must have been in a situation to be seen from some part of the earth. It must be recollected that this comet can never have come to its perihelion in the months of November, December, January and February, without being a conspicuous object in the morning or evening twilight, before or after the passage of the perihelion. In all instances it must have been best seen in the southern hemisphere. We have given nearly all the coincidences in dates. Those which have no (*), nor (?) annexed are coincidences in date. Those marked with an (*) have, besides the coincidence in date, some circumstance, whether of physical appearance or apparent path in the heavens, analogous with the comet of 1843. Those marked with an (?) are probably mere coincidences in date without being the same individuals.

| Date. | Periods of eight revolutions preceding the recent appearance. | Single revolutions and mean period. |
|---|---|---|
| B. C. 432 | 13×175.00 | 104×21.876 |
| A. D. 268 (?) | 9×175.02 | 72×21.878 |
| 442 (?) | 8×175.15 | 64×21.894 |
| 617 | 7×175.17 | 56×21.896 |
| 968 | 5×175.04 | 40×21.880 |
| 1143 (*) | 4×175.05 | 32×21.881 |
| 1317 | 3×175.40 | 24×21.925 |
| 1493 | 2×175.10 | 16×21.888 |
| 1668.2 (*) | 1×175.00 | 8×21.875 |
| 1843.2 | | |

| Date. | Periods of eight revolutions preceding expected return in 1865.04. | Single revolutions and mean period. |
|---|---|---|
| B. C. 60 (*) | 11×175.00 | 88×21.875 |
| A. D. 290 (*) | 9×175.00 | 72×21.875 |
| 639 (!) | 7×175.14 | 56×21.892 |
| 815 | 6×174.99 | 48×21.874 |
| 990 ? | 5×174.99 | 40×21.874 |
| 1165 | 4×175.23 | 32×21.904 |
| 1340 * | 3×174.98 | 24×21.873 |
| 1516 ? | 2×174.47 | 16×21.809 |
| 1689.95(*) | 1×175.00 | 8×21.875 |
| 1865.04 | | |

| Date. | Periods of eight revolutions preceding expected return in 1886.91. | Single revolutions and mean period. |
|---|---|---|
| B. C. 213 | 12×174.99 | 96×21.874 |
| A. D. 488 | 8×174.85 | 64×21.856 |
| 837 | 6×174.97 | 48×21.871 |
| 1012 (*) | 5×174.96 | 40×21.870 |
| 1362 (*) | 3×174.94 | 24×21.867 |
| 1537 (*) | 2×174.91 | 16×21.864 |
| 1886.91 | | |

We have stated the opinion of several able astronomers, that the densest portion of the nebulosity of the recent comet, necessarily selected as the proper point for micrometric measures, was eccentric towards the sun from the real head or centre of gravity of the comet, tail, and nebulous envelope. In fact the comet never presented any appearance of a distinct kernel or head, but only a vague and ill-defined nebulosity or cloud, gradually condensed towards the centre, or, according to Messrs. Herrick and Bradley, towards a point nearer the sun than the centre of the disc, if we may so call it, of the nebulosity. In Prof. Bartlett's letter, mention is made only of the elastic force of the vaporous matter surrounding the comet and composing its envelope or tail. On this hypothesis, we have suggested the simplest and most natural method of completing the elliptic elements; viz. that of making the excess of the supposed elliptic, over the actual hyperbolic radius vector of the point observed, equal to a constant coefficient of the reciprocal of the square of the radius vector; and determining by means of the constancy of this value, the actual eccentricity corresponding to a period of $21\frac{7}{8}$ years, and a perihelion point of the centre of gravity or head of the comet actually outside of the sun though nearly in contact with it. In fact this multiplier is not necessarily constant, nor necessarily a co-efficient of the reciprocal of the square of the radius vector; still this hypothesis is the most simple and plausible that can be made, and is perhaps quite as complex as the nature of the question permits us to make.

As the subject of the physical organization of the head, tail, and nebulosity or envelope of comets, has been discussed by Sir William Herschel, Olbers, Brandes and Bessel, with their characteristic genius and acumen, we deem it proper to consider the bearing of their opinions and researches on the present question.

Sir William Herschel* states that the kernel or head of the great comet of 1811, of about 1″ in diameter, could only be seen with high powers in his most powerful telescopes, and that with ordinary instruments he saw only the nebulosity or envelope; but that when the head or kernel was seen, it was seen within the envelope eccentric from the sun, or in other words the densest portion of the envelope or nebulosity was eccentric towards the sun. This is precisely the phenomenon observed by Messrs. Herrick and Bradley with reference to the disc of the nebulosity, though the kernel or head could not be seen. This also agrees with Bessel's remark, that this comet seems to have thrown out nearly all its head in forming the nebulosity and tail.

We come next to Olbers's theory† of the formation of the envelope and tail of comets. This was promulgated in 1812, shortly after the appearance of the great comet of 1811. We do not recollect to have any where met with a translation of it. It is perhaps the only theory ever proposed, that explains all the phenomena observed respecting that comet. Olbers supposes that any particle composing the surface of the comet, or approaching from the frozen regions of space within a certain distance of the sun, is affected with a new repulsive force, resembling that which drives off substances from an excited prime conductor. These particles, thus polarized, he supposes to be thrown off from the head of the comet with a force proportioned to the mass of this head or nearly solid portion of the comet, and inversely as the square of the distance from the centre of this head. The same particle in acquiring polarity with reference to the comet, acquires also polarity with reference to the mass of the sun, and is repelled by that

* Monatliche Correspondenz, Vol. XXVIII. p. 459.

† Ibid Vol. XXV. p. 3. Prof. Norton's theory of comets' tails, read at this meeting, much resembles Olbers' theory.—S. C. W.

mass, instead of being attracted by it with a force also varying inversely as the square of the distance from the sun. The origin of this polarity may be ascribed to the action of the sun's light or heat, or both. This particle, thus endued with one repulsive force, acting in the direction of the prolongation of the radius vector from the sun, and inversely as its square, and with another repulsive force, acting in the direction of the prolongation of its radius vector from the centre of the comet, and inversely as its square, and with its original tangential velocity, at the time of parting with its actual cohesion with the comet, moves away in space in such a manner as not to return. Now, the geocentric position in the heavens with respect to the head of the comet, of any such particle, for any given elapsed time after it is thrown off from the comet's surface, may be readily computed, from the known tangential direction and velocity for any assumed values of the two repulsive forces of the comet and sun, for a unit of distance—say the earth's mean distance from the sun. Olbers remarks, that the heliocentric orbit of such a particle must be an hyperbola; moreover, that the points in space where the two repulsive forces of the sun and comet make equilibrium, for any original direction of repulsion from the surface of the head, must have the portions of expelled matter more condensed than any portion of space between such points and the comet's head, thus forming, apparently, a hollow envelope or nebulosity, in the shape of an hyperboloid, having the head of the comet in its internal focus, and its apex towards the sun; the continuation of this hollow hyperboloid from the sun, beyond the parameter, so to speak, forms the tail of the comet. The shape of the hyperboloid, and, consequently, of the tail, depends upon the ratio of the repulsive forces of the sun and comet. If the one is determined by measure, the other can be computed from it. Olbers made measures of the shape of the visible section of this hyperboloid.

Brandes* gave this theory a thorough discussion, and finds analytically that the opinion of Olbers is true, that the envelope, if so caused, must be an hyperboloid; and then, from the observed dimensions of a section of this hyperboloid, as seen from the earth, computes the ratio of the two repelling forces for that comet.

The theory of the formation of comets' tails seems to have made but little advances from 1812 till the return of Halley's comet in 1835; when the astronomers of Europe, with a full knowledge of Ol-

* Monatliche Correspondenz, Vol. XXVI. p. 533.

bers's theory, employed their powerful instruments to observe the tail and nebulosity of that comet with reference to it.* Struve remarked, that the densest point of the nebulosity was eccentric in that nebulosity, conformably to the observation of Messrs. Herrick and Bradley for the recent comet. But the most indefatigable observer was Bessel,† with the Königsberg heliometer. He detected a pendulous or vibratory motion of that portion of the nebulous matter which was expelled from the hemisphere of the comet next the sun, resembling that of a magnet round the magnetic pole. He finds, that with the addition of this pendulous motion of this streaming matter, or, in other words, of the apex of the hyperboloidal envelope of the comet in the plane of the orbit, and for fifty degrees or more on each side of the antipode of the comet's tangential direction, all the observed phenomena of the tail of Halley's comet may be explained. Bessel then proceeds to compute the repulsive force of the sun and comet on these expelled particles, from the observed shape of the tail. He also computes, for any point of the tail, how long the expelled particle has taken, since the date of its expulsion, to arrive at the point observed.

All these phenomena Bessel explains upon the supposition that there is no molecular attraction or repulsion between the particles thus expelled; but that their heliocentric motion is due to the tangential direction in which they are expelled from the comet, and to the two forces of repulsion of the sun and comet, and the orbital direction and velocity of the particle at the time of expulsion. The reason assigned by Bessel for not supposing a molecular connexion between the particles of the nebulosity of the comet, is the fact, that the rays from a star seen by himself in the Königsberg heliometer, through this nebulosity, were not refracted by the medium of the nebulosity, but preserved, when seen through this medium, the same relative position with reference to any other fixed star, as when seen before entering the medium. Bessel argues that this could not have been a vapour, gas, or air, through which the star was seen, or it would have been refracted.

Having stated the principal points of the theories of Olbers and Bessel regarding the formation of comets' tails, it remains to consider their bearing on the present question. It is obvious that the

* Schumacher's Astronomische Nachrichten, Vol. XIII. p. 303.

† Ibid. Vol. XIII. p. 177. Also Schumacher's Jahrbuch for 1837, p. 142. Con. des Tems, 1839.

difference between the radius vector of the head, and of any particle of the nebulosity, one in the point aimed at for instance, would not be a simple multiplier of the reciprocal of the square of the radius vector, but would, on the contrary, include a term depending upon the reciprocal of the square of the distance of this particle from the centre of the head. It may be shown, however, from the length and narrowness of the tail of the recent comet, that this latter term is nearly insensible, since the comet's expulsive force, compared with the sun's, must in this instance have been very small; for when acting in the normal to the radius vector, it was able to impress but a small normal velocity on the particles thrown off.

We conclude then, that whatever theory of the formation of comets' tails we adopt, if we suppose the particles of the nebulosity and tail to be material, and the densest portion of the former to be extended from the centre of the comet's head towards the sun, the simplest method of deriving the true elements of the orbit of the head, from the computed elements of the orbit of the point observed, is that which we mentioned in our letter of the 25th of May last.

We subjoin the elements of the comet computed from our first normal places, by Professor Anderson, of Columbia College, New York, viz.—

| | | |
|---|---|---|
| Perihelion passage, | Feb. $27^{d}.579857$ m. t. Green. | |
| Longitude of perihelion, | 279° 40′ 35″.50 | m. eq. March 30. |
| Long. of ascending node, | 15° 0′ 56″.45 | |
| Inclination, | 34° 21′ 6″ | |
| Eccentricity, | 1.0008560 | |
| Gaussian angle, | 2° 23′ 0″.76 | |
| Perihelion distance, | 0.00415697 | |
| Mean daily motion retrograde, | 146″.50299 | |

Lieut. Gilliss read a communication, entitled, "History of the progress in establishing an Observatory at Washington City, Description of the Building erecting, and of the Instruments ordered for the Depôt of Charts and Instruments of the U. S. Navy, by Lieut. J. M. Gilliss, U. S. N."

In this paper, the author gives a succinct history of the depôt of charts and instruments under the charge of a bureau of the Navy Department, and describes the site which has been designated by the President of the United States for the construction of the buildings hereafter to be used as the depôt.

The site designated by him is on the north bank of the Potomac, in the south-western part of the city of Washington; the plot containing a little more than 19 acres. Its north front is about 790, the east 1150, and west 700 feet long; and the spot selected for the building is at the intersection of the axes of two streets prolonged, one being in the meridian, the other in the prime vertical. This point is 100 feet above tide-water, has a north horizontal range one and a quarter, and south range eight miles, and is 105 yards from the east, 95 yards from the north, and 300 yards from the south enclosure, the last being the river. The hill is of gravel formation, with a surface stratum of dry brittle clay, through which water filters almost as freely as through gravel. An excavation eight feet deep has been made for the walls and piers of the entire building; and it is intended, for greater security, to leave a ditch ten feet wide by nine feet deep, surrounding the observatories.

The author then describes with minuteness the plans of the astronomical depôt and the magnetic observatory, and the manner in which it is proposed to execute them. He remarks that these were submitted by him to several of the most experienced European observers, and that they have met their approval. The paper concludes with a particular description of the great achromatic refractor, comet seeker, meridian transit, mural circle, transit for prime vertical, and the magnetic and meteorological instruments, which have been ordered in Europe for the use of the depôt. It is accompanied by drawings of the ground plan and elevation of the two buildings.

Mr. William C. Redfield of New York, read a communication "On Tides, and the Prevailing Currents of the Ocean and Atmosphere."

Mr. Redfield stated that the substance of this paper had been prepared in 1838, at the request of a gentleman attached to the United States Exploring Expedition, together with a series of maps and charts on which were delineated the predominating systems of winds and currents in different oceans, derived from the log-books of voyagers and other sources. These maps had been lost, in the wreck of the Peacock, but the general remarks and statements which had accompanied them he now submitted to the Society.

In his remarks on tides, Mr. R. suggested the importance of good observations on the direction of the stream of flood tide in the *offings* of the islands and headlands visited by the expedition, especially in positions not exposed to the local influence of banks and shallows.

He also suggested the inquiry whether the great oceanic tide-wave does not perform a great revolution, on each side of the equator, in each great ocean basin, moving westward in the intertropical latitudes and returning eastward in the higher latitudes; if, indeed, this question was not deemed already settled by the inquiries of Professor Whewell.

In treating of the great oceanic currents, Mr. R. noticed the course of revolution in the gulf-stream system of currents in the North Atlantic, and the counter-revolution of the northern offset from this system, which supplies in part the great polar current that sweeps along the coasts of Labrador and Newfoundland. It has been alleged by late writers, that this polar current passes from the shores of Newfoundland to those of Europe, in coincidence with the gulf stream; but Mr. R. maintained that its southerly course is continued along the American coast and into the ocean depths of the Atlantic, and that at its intersection with the course of the gulf stream it becomes mainly a subaqueous current. The proofs of this he found in the impelling effects of the deep polar current upon the icebergs which it drives into the gulf stream, or even across it, unless sooner dissolved by the warm superior current from the tropic, and in the reduced temperature of the more sluggish and diffused portion of the polar current, which continues its south-westerly course along the coast of the United States, to the westward of the gulf stream.

Systems of currents analogous to the foregoing, and proceeding from similar causes, Mr. R. described as prevailing in other oceans, but modified and controlled in their courses by the direction and contour of the continental coasts, and by the islands and extensive coral reefs of the Pacific. Thus, his inquiries had shown a warm current in the western Pacific, corresponding to the gulf stream of the Atlantic, and which had been incidentally noticed by the officers of Cook's third expedition, as running on some occasions with a velocity of five miles an hour.

After some allusions to the geological effects of the permanent systems of ocean currents, Mr. Redfield proceeded to notice the predominating currents of the atmosphere, as exhibited in the unceasing and mainly horizontal movements of the air, in circuits or systems of revolution and compensation, in the several oceanic basins of the earth's surface, on both sides of the equator. Of these systems of circulation and revolution, which bear a general resemblance to those of the ocean currents, the portions which move westwardly in the lower latitudes constitute the so called trade winds; while the opposite winds, or counterparts of the several circuits, are found in the more gyratory and irregular winds of the temperate zones, which maintain a general movement to the eastward, that fully compensates for the westwardly flow of the trade winds. The immediate but often unseen connexion of these opposite winds in continuous parabolic or elliptic circuits, of varying forms and extent, is more conspicuous in some regions than in others, and is most clearly developed near the extreme eastern and western borders of an ocean basin; as in the region which includes the island of Madeira and the Canaries, in which northwardly winds are found to prevail; while near the western margin of the same ocean, in like latitudes, southerly winds are most often met with, except in storms or at particular seasons.

The major axes of revolution, in these great natural circuits of wind, are generally found in those extensive belts or regions of extra-tropical calms and light winds, which are found in all the great oceans, and which, in the North Atlantic, are known among mariners as the " horse latitudes." A more extensive belt of calms and light winds, as is well known, generally separates the trade winds and revolving systems of the two hemispheres, in the regions near the equator.

The generally horizontal winds of our globe, as thus developed in the great systems of circulation in its atmosphere, Mr. Redfield had referred on some former occasions to the law of gravitation, as connected with the rotary and orbital movements of the different geographical parallels and meridians of the earth's crust; the nearest attainable equilibrium of the fluids which envelop the planet being that of motion, not of rest. But a different theory of winds has been generally adopted, founded on the alleged effects of rarefaction in the equatorial regions. Without discussing these theories, and while admitting heat as a cause and modifier of winds, to some considerable extent, Mr. R. now alleges the following as valid and insuperable objections to the common theory of the trade winds, viz.

1st. The specific difference of temperature in the intertropical winds, as compared with equal zones of extra-tropical winds, is inadequate and disproportioned to the dynamical effects exhibited in these winds.

2d. The ascent of the body of trade wind to the upper atmosphere, in the equatorial latitudes, has never been shown by observation, and may well be denied.

3d. The perpetual snow line of the Andes is found to be near 1000 feet higher in 16° to 18° south latitude than at the equator or on the parallels of the equatorial calms.

4th. The semi-annual change in the locality of the zone of greatest heat is not productive of any like degree of change in the locality of the trade winds.

5th. The course of the winds in extensive portions of the torrid zone appears wholly irreconcilable with the received theory.

6th. In our American summers, the hottest winds are often found moving horizontally on the earth's surface, for several successive days, but not towards the equator, nor rising from the surface, as the theory requires.

Further proofs of the horizontal course of revolution in the trades and the general winds, in opposition to the calorific theory, was found in the known progress and routes of those extensive portions of the lower atmosphere which comprise the great storms, as shown by the inquiries of himself, Col. Reid, and others. These extensive portions of atmosphere cannot be supposed to possess any self-moving power or tendency other than gravitation, but must move in accordance with the predominating physical impulse to which the lower atmosphere is subject in the regions through which the storm may pass.

The *rotation* of these storms, also, in a determinate direction, proves the generally horizontal course of the winds on the earth's surface, and the dynamical influences of the earth's rotation on the atmosphere; for it can be shown by an experiment which Mr. R. pointed out, that a tendency to gyrate from right to left in the northern hemisphere, and in the counter direction in the southern, is necessarily imparted to the surface winds, which move from the equator towards the temperate latitudes, in the great circuits of revolution which he had ascribed to the trades and to the general winds.

Dr. Horner, Professor of Anatomy in the University of Pennsylvania, presented a summary view of the existing application of the microscope to human anatomy and to animal organization generally, and considered many of their important problems as solved by its recent improvements.

He alluded to the observations of the earlier microscopical anatomists, and the difficulties they experienced from the want of an adequate provision against spherical and chromatic aberration. These difficulties had at length been surmounted, so as to afford greater uniformity between different observers, and thereby to produce greater confidence in the results. Many points may, indeed, be considered as definitively settled. He spoke highly of the compound microscope of Ploss, but gave the award of decided superiority to that of Powell and Lealand, of London, which had been introduced to the notice of the citizens of Philadelphia, by Dr. Ch. Fr. Beck.

The points of microscopic anatomy, to which he referred specially in illustration of his views, were the settlement of the questions on the diameter and structure of the blood discs or corpuscles, on the chyle and lymph corpuscles, on the fat vesicles, and on the texture of the cuticle and epithelium.

He also alluded to the discovery of Schwann, that the rudimentary state of every texture and organ is that of cell, and that from its modified evolution every thing else was developed; a principle in histogeny more prolific in consequences than any other which had ever been asserted.

He likewise alluded to the cellular origin of the colours of different parts of the human body, that they were all, including that of the skin and choroid coat of the eye, generated in pigment cells.

He introduced, further, a notice of the microscopic organization of the nails, teeth, muscular tissue, and mentioned the point, now decidedly ascertained, of the nervous fibrillæ running their course from origin to termination without anastomosis, and of their being in the condition of tubes.

In this limited notice of the innumerable objects of microscopical observation, he considered himself as doing very inadequate justice to the labours of the gentlemen who had enlarged so considerably the boundaries of human knowledge; but the technical character of the subjects compelled him to make only a brief exposition of what had been done: his chief notice being to show, that while mathematics, natural philosophy, natural history, and chemistry, are pushing forward their confines, anatomy has not been idle.

Professor Bache presented the results of two years' observations of the magnetic elements, and of the temperature, pressure, and moisture of the atmosphere, at the Magnetic Observatory at the Girard College.

The Observatory was opened in May, 1840, and the results are for two years from that time. Prof. Bache, after alluding generally to the different results obtained from the magnetic observations, called particular attention to the averages of the hourly changes. The observed elements were the declination, and the horizontal and vertical forces. The changes of each of these were represented by curves in the usual way, the differences of the abscissæ representing the times elapsed, and the differences of the ordinates the change of element; increasing ordinates corresponding to increase of declination or of force. The curves, even with the limited number of observations upon which they are founded, are remarkable for their regularity.

The declination shows distinctly two maxima and two minima during the twenty-four hours. The chief maximum is about 1 P. M., and the subordinate maximum about 1 A. M.; the latter, though marked, does not reach the mean line of the twenty-four hours. The minima are at about 8 A. M. and 9 P. M. The mid-day maximum was about 8′ above the preceding, and 6′ above the succeeding minimum, and the midnight maximum 1′ above the next preceding, and 3′ above the next succeeding minimum. The line of mean declination crosses the curve about 10 A. M. and 6 P. M. The force observations were, Prof. Bache remarked, corrected approximately for temperature; and he made some statements to show how difficult it was to obtain a correction for temperature, especially in large magnetic bars. He expressed himself not satisfied with the correction yet obtained, although it resulted from a very large number of comparisons, and appeared to represent reasonably well the average correction.

The curves representing the horizontal and vertical force observations were traced separately. The horizontal force shows, like the declination, two tides in the twenty-four hours; the vertical force but one. The maxima of the horizontal force are at about 6 A. M. and 2 P. M., and the minima about noon and 9 P. M. The maximum of the vertical force is at about 1 P. M., and the minimum about 9 P. M. Prof. Bache remarked upon the changes of total force and dip, as indicated by those of the horizontal and vertical force. The line of mean horizontal force crosses the curve four times, at about 8 A. M., 2 P. M., 5 P. M., and 11½ P. M. The line of mean vertical force crosses the curve twice, at about 6 A. M. and 6 P. M. The range of horizontal force, or difference between the greatest maximum of the twenty-four hours and the lesser minimum, is about .001,092, expressed in parts of the entire horizontal force; and the same difference for the vertical force is about .001,024. These observations had been made the basis of a series to determine the times of maxima of the several elements more nearly, by observations taken at short intervals, at the periods of the day including these times.

Prof. Bache gave reasons why he considered the phenomena thus presented to be inconsistent with the idea, that they are produced by the heating effects of the sun, acting either directly or indirectly.

The averages of meteorological observations were of the thermometer and barometer; the force of vapour also was given as deduced by Prof. Apjohn's formula and the tables in the instructions of the Committee of Physics of the Royal Society, from the observations of the wet bulb hygrometer. The hourly means for the two years, and the means for each month, were represented by curves. The curve of hourly change of temperature was of the parabolic form frequently before obtained, with irregularities depending upon the number of observations. The periods of maxima and minima were about 3 P. M. and 5 A. M. The curve representing the force of vapour resembles, generally, that of temperature, though it is by no means as regular; the maximum being however about 6 P. M., and the minimum between 4 and 5 A. M. The curve of pressure shows the two atmospheric tides, the maxima at about 9 A. M. and midnight, and the minima at 2 A. M. and 5 P. M. The mean barometric fluctuation in twenty-four hours is about .06 of an inch. The similarity of the curves of monthly means of temperature and force of vapour is even greater than that of the corresponding curves of hourly means. The maximum temperature is reached about the middle of July, and the maximum force of vapour later in the same month; the minima in December and January. The curves of pressure prove, that an element besides the temperature and force of vapour must enter into its composition. Comparisons of the numbers for the force of wind had not yet been made, though the data had been obtained.

Prof. Bache observed, in conclusion, that as the Observatory had been re-opened by aid obtained from the Secretary of War, similar observations would by degrees be accumulated, and by an increased number of results render the means more trustworthy than those now presented.

At the close of his remarks, Prof. B. invited the members and correspondents of the Society and the gentlemen attending its meetings, to inspect the Observatory at the Girard College after the adjournment in the afternoon.

Mr. Henry D. Gilpin, pursuant to appointment under Chapter I., Section 13, of the Laws, presented a Biographical Notice of the Hon. Edward Livingston, late a member of the Society.

Mr. Gilpin introduced the designated subject of his paper by claiming the honours of distinguished membership in our Society for all who, whatever their pursuits of life, have illustrated our country by their intelligence, or happily guided it by their wisdom. Without detracting from the merit and the praise of those, who in the pursuit of natural or exact science and the elucidation of moral truths, more usually win, in the general estimation of the world, the cherished titles of philosophy; no one, he said, does philosophy claim more justly and truly as her son, than him, who in the active engagements of a public career, where he is stimulated by ambition, and occupied by objects that are supposed most strongly to absorb the feelings, if not to warp the judgment and the taste, yet blends with all his actions the love of science and the extension of truth, and applies that wisdom which springs only from knowledge and truth, to the affairs he is called upon to engage in or direct. Exemplifying this observation by the names of Aristotle, Cicero, and Bacon, and by those of Jefferson and Franklin, Mr. Gilpin asserts for that of Mr. Livingston a place in the same category with theirs. He points to the dignity of his intellectual labours, the light which they reflect of philosophic thought and large experience, their eminent practical value, and the consistent anxiety which they always evince for the advancement of human happiness and virtue. With a mind, he says, clear, penetrating, and sagacious; with an industry that left unfinished no duty that he undertook, there was blended from his earliest youth a serenity of temper, simplicity and cheerfulness of manners and active benevolence, a clear strong sense of right, a desire to promote in all things the good of others, and a willingness to forego his own interest and inclinations: so that in all the relations of an active and varied life, he filled his part, not more for his own enduring reputation, than for the benefit of those he served.

Born but a short time before the commencement of the revolution, the youthful years of Mr. Livingston were impregnated with the lessons best taught to an observing mind by the incidents that occurred around him. A brother of Robert R. Livingston, one of the Committee who draughted the Declaration of Independence; a brother-in-law of Montgomery, who sealed with his blood the manifesto of patriotic resistance; filled with an insatiable love of study, by which he had mastered the stores of ancient and modern learning, and acquired a knowledge, far from inconsiderable, of many of the branches of abstract and natural science; he came into life just at the period when the institutions of his country assumed their settled form, imbued with the true spirit in which they were founded, animated with the desire to maintain them in purity and vigour, and possessing the talents and information which would enable him well to perform his part, in whatever situation he might be thrown, as a private or a public man.

Having adopted the legal profession, and established himself in the City of New York, he had gained before he reached the age of thirty, a high reputation for the extent of his acquirements as a jurist, and ability as an advocate. He already began to apply to his professional investigations the principles which he had accumulated in the wide range of his legal studies, and in some degree introduced into a practice, necessarily founded upon and nearly confined to the English law, those illustrations which he had derived from the jurists of antiquity and of continental Europe, and which at a subsequent period were so conspicuously and advantageously exhibited in his public and professional labours. From these occupations he was in some degree withdrawn for several years by his election to Congress, as a representative from the City of New York. That event took place in the year 1794. He was twice reëlected; and during the six years that he remained in Congress, he maintained a position equally distinguished by the ability that marked his views on all public questions, and the enlightened and candid spirit which he evinced in the discussions of a period, when those differences were first developed, that presently assumed a character more ardent and limits more distinctly defined. United in political opinions with Madison, Gallatin, Giles and Macon, he bore a conspicuous share in the debates on the public measures which they advocated or opposed; and came at once to be considered as a leading member of the party to which he attached himself. Independently, however, of subjects more peculiarly political, he was early the advocate of various measures indicative of a wise and philanthropic spirit; and among these, it is especially to his exertions that we owe the first endeavours to reform the criminal code of the United States, to protect or relieve American seamen left by accident or misfortune on foreign shores, and to promote the gradual increase of a navy sufficient to protect American commerce in remote seas. Though sincerely attached from a strong conviction to the particular political opinions which he advocated, he yet maintained them with characteristic liberality and deference to those from whom he differed, and no diversity of views of general policy could induce him to withhold his cordial support to such of their measures as he deemed calculated to sustain or protect his country's honour or rights.

Soon after withdrawing from Congress, he was elected Mayor of the City of New York, an office, whose organization as then constituted required the exercise of important judicial as well as executive functions. He was also selected by Mr. Jefferson, when he became President, to fill the post, for which his legal acquirements eminently fitted him, of Attorney of the United States for the State of New York. During the period of his mayoralty, the city was afflicted by a desolating pestilence; during which his personal exertions and benevolence were fearlessly displayed at the risk, and almost with the loss of his own life. The Common Council, on his subsequent retirement from office, adverted, in an address unanimously adopted, to his admirable conduct in that trying emergency and in the whole performance of his public duties, and expressed to him the gratitude of the community in glowing terms.

When Louisiana had been gained for the United States by the diplomatic labours of his brother the Chancellor, Mr. Livingston resolved to remove there, and to connect his renewed professional career with the rising institutions of the new community. The enlarged nature of his earlier legal studies, enabling him at once to grasp the questions which arose out of the provisions of the civil law, as well as that of France and Spain, introduced there at different periods of colonial authority; his thorough knowledge of the jurisprudence then generally prevailing in the United States, which would necessarily come to be incorporated to some extent with that of a territory now a part of them; and above all his habit and power of careful discrimination of legal principles, looking to them according to their intrinsic excellence and fitness, neither in a spirit of unnecessary innovation nor an unwise adherence to mere precedent or usage; these qualities not only placed him at once, by general consent, at the head of his profession in Louisiana, but they enabled him to exercise a more than common influence in establishing a system of jurisprudence there, which in all respects may bear an advantageous comparison with that of any other of the states, and may claim over that of many of them a decided superiority.

Immediately on his arrival in Louisiana, Mr. Livingston perceived the necessity of prompt attention to this subject. The inhabitants had grown up and were living under a system of laws, which were henceforth to be administered by judges, some of whom were ignorant of the languages in which they had been promulgated, and most of whom had been accustomed to judicial forms altogether different. On the other hand, the institutions of a free people were to supplant those of a monarchical and colonial government, and the individual citizen was to be called on constantly to perform a part personally active and efficient. It became therefore at once essential that, even without a change in the body of the laws, a mode of procedure fitted to the circumstances should be established without delay. The legislature wisely resolved to commit this duty to the judgment and knowledge of jurists, in whom the people might safely repose unlimited confidence. Mr. Livingston was selected to perform it, and with him was united a personal and professional friend of learning and ability, also while he lived a member of our Society, Mr. James Brown, afterwards a Senator in Congress from Louisiana, and Minister Plenipotentiary to France. While they discarded the fictions and technicalities of the English law, they avoided the prolixity so usual in the Spanish, and not infrequent in the French code. Their system is simple and intelligible, well calculated to prevent unnecessary expense and delay. It was adopted by the legislative council; it was introduced with the general approbation of the community; and for a series of years it has stood, with slight alteration, the test of trial and experience.

A more important task remained—the complete revision of the body of civil and criminal law; and its reduction into systematic codes. This was naturally and properly postponed until, by their admission into the Union as a sovereign state, the people could themselves act upon a measure so important to their feelings and welfare. After this event, with a just estimate of the wisdom and ability of Mr. Livingston, he was selected for the task by the Legislature: no difference of opinion upon the political topics of the day withdrew their confidence from one, who had identified his fame with the jurisprudence of Louisiana, as he had devoted his talents to their service. In the preparation of the civil code, Mr. Dubigny and Mr. Moreau were united with Mr. Livingston. The task proved to be one of great labour: the existing laws, which were familiar to the people and therefore not without necessity to be abolished, consisted of provisions at once complicated and discordant; the fragments of the Spanish ordinances frequently remained; the French law previous to the revolution had not been altogether superseded by the code of Napoleon; and, with American judges, and the influx of American citizens, many of the provisions of the English common law had obtained a place. The arduous exertions of three years were required to reduce this mass into an intelligible shape. In all its parts it received the coöperation of Mr. Livingston; and some of them, especially the title of "obligations," were exclusively his own. It met with a reception from the Legislature and people of the state far more favourable than could have been anticipated for such a measure; and with the exception of the commercial code, to some provisions of which objection was made, it was promptly adopted and still continues, with few alterations in its general principles, to be the permanent law of the state.

By an act of the Legislature the preparation of a system of criminal jurisprudence was confided to Mr. Livingston alone. He deeply felt the responsibility he assumed in undertaking such a trust; he knew that he would have to encounter strong prejudices, to oppose long settled opinions, to exercise a vigilance of forecast and distinctness of enactment as to the objects, for which the state of society present and future required him to provide. Two years after his appointment, he presented to the Legislature a preliminary report, exhibiting the progress he had made, explaining the plan on which he proposed to execute the work, and giving some detailed parts as specimens of it. These were unanimously approved, and he was requested by a vote to complete his labours. He accordingly proceeded with it. His best faculties, to use his own language, were faithfully and laboriously employed, under the direction of a religious desire to perform the duty entrusted to him in a manner that might realize in some degree the views of his fellow citizens, for whose benefit it was designed. By assiduous exertion he completed the entire work in two years more; but it was scarcely finished when all his labours were destroyed by an accident, that fortunately, in its final result, only produced a remarkable instance of his equanimity and perseverance. Having received authority from the Legislature to submit it to them, when completed, for greater convenience in a printed form, he had caused a fair copy of the whole work to be written for the use of the printer. The evening before it was to be delivered to him, he occupied himself till a late hour in comparing this copy with the original draught. He left them together when he went to bed, consoling himself with the pleasing thought that he had thus completed the labours of four years. Not long afterwards he was awakened by the cry of fire; he hastened to the room where his papers had been left, but not a vestige of either copy remained. They were totally consumed. Though stunned at first by the event, his industry and equanimity soon came to his aid: before the next day closed, he had recommenced his task; the Legislature at their following session extended the period for its performance; and in two years more, he presented to them his complete "System of Penal Law," in the shape in which we now see it. Prefixed to the system was a series of reports, reviewing in a masterly manner the whole science of penal jurisprudence; pointing out the objects to be sought for, the errors to be combated, and the modes in which these could be done with most benefit to the criminal himself and to the society whose laws he had violated. The system has not, it is believed, been yet finally acted upon, in its extended form, by the Legislature of Louisiana; but it does not on this account claim less justly the admiration of the philanthropist and jurist. It is a work worthy of the deep consideration of all communities. The beauty of its arrangement, the wisdom of its provisions, and the simplicity of its forms, have never been surpassed, probably never equalled in any similar work; and it is not without entire justice that this admirable production has contributed, perhaps more than any other of his labours, to secure to Mr. Livingston that eminent place which he holds among those who are regarded not merely as distinguished jurists but as public benefactors.

It was by these acts, during an uninterrupted residence of many years, that Mr. Livingston identified himself with the state of which he became a citizen. His name will ever be cherished with grateful affection and respect in Louisiana. Nor was it by these acts alone. His eminent standing in his profession and in society, the active interest which he took in all the institutions of the state, and his services in the Legislature, of which he was occasionally a member, all united to make him not only an influential citizen, but one who was able in innumerable ways to contribute largely to the benefit of the community. His patient industry, his amenity of temper, the generosity of his disposition, made this at once easy and agreeable; and when, in the circumstances of the times, acts of more serious devotion to public duty were required, he was found amongst the foremost, ready and zealous to discharge them. The invasion of the British at the close of the war roused the patriotic spirit, as it required the prompt devotion of the inhabitants. With but few regular troops, and almost entirely unprepared for such a conflict, they were obliged hastily to form themselves into an army to repel the invaders. Mr. Livingston was among the foremost to do so. Instantly leaving his professional duties and all private occupations, he presented himself to General Jackson as soon as he arrived to take the command in Louisiana, and offered to place himself in any position where the General might regard his services as useful. He was selected as his aid-de-camp. He was by his side constantly throughout the period of hostilities, enjoyed his confidence in a marked degree, and at the close of the war received from him many evidences of that regard which was afterwards, and in another station, yet more signally displayed.

After an uninterrupted residence in Louisiana for twenty years, in which he had withdrawn from political pursuits, and devoted himself to his profession and those congenial studies and labours that have been adverted to, Mr. Livingston determined to retire from the bar, and to revisit in New York the scenes of his earlier life, and the connexions from whom he had been so long separated. This determination was the signal for a new mark of confidence from his adopted state. He was elected as a representative in Congress from Louisiana, an event which was followed in a few years by his choice as a Senator. After his election, an enthusiastic address was presented to him by the City Council of New Orleans, in which they reviewed his various public services from the moment of his arrival in Louisiana, spoke of them in warm terms of approbation and gratitude, and expressed their confidence that his continuance in the national councils would be a sure guarantee of further exertions for their welfare and prosperity.

Mr. Livingston continued in Congress from 1823 to 1831. His advanced age prevented the same energetic participation in the public business which had there formerly distinguished him, but he nevertheless, originated several important measures, and not unfrequently engaged in debate. The speeches which have been preserved exhibit that clearness of perception and language, that various but unostentatious learning, that simplicity, dignity and patriotism, which were characteristic of him. His views of public questions were expressed with firmness, but without asperity: he discussed, in a masterly manner, all those topics connected with a true construction of the constitution, and the extent and limitations of power assigned to the members of the confederacy and to the different departments of the government, which grew out of the controversy in South Carolina: the reputation which he had acquired in Congress so long before, and that which had been added to it by his eminent labours in a different sphere, suffered no diminution, but gained additional lustre by his return to a legislative career.

In the spring of 1831, the Department of State became vacant by the resignation of Mr. Van Buren. Mr. Livingston, who had retired a few months before, at the close of the session of Congress, to an estate which he possessed on the Hudson river in the neighbourhood of his birth-place, was summoned by General Jackson to fill that elevated post. Totally unprepared for such an event, he hesitated for some time to accept it: with the modesty and simplicity which marked his character, he distrusted his abilities adequately to discharge its duties; and it was not without difficulty that the President obtained the services of one, whose devotion to his country he had himself witnessed in far different scenes, and whose talents and virtues had received the approbation of his countrymen so often, and in so many ways. Eminent as have been the men who have filled the post of Secretary of State, few have displayed the same fitness and ability to discharge its duties. His negotiations with foreign nations were very successful; and the documents connected with them, so far as they have been published, exhibit profound political wisdom, and an enlightened spirit. The treaties that he formed are not more beneficial in their commercial stipulations, than they are made consonant, in their international provisions, with the feelings and improvement of the age. The missions which he originated or promoted, have opened new and important fields to American enterprise. The counsels of which, as the chief member of the administration, he was the advocate or adviser, were founded on views of the constitution carefully considered and ably vindicated.

The duties of such a place were however more arduous than Mr. Livingston, at his advanced age, was willing to continue long to discharge; and on the re-election of General Jackson in 1833, he retired from his cabinet. At that time the negotiations with France, arising out of the treaty of 1830, which granted an indemnity to the United States for injuries done to American commerce during the wars of Napoleon, were in a state of great complexity. This was increased by the excitement which party contests in the French legislature gave to the subject; and it was evident that the position of affairs demanded such a course on the part of the United States as should protect its honour and maintain its rights, without allowing any thing, not required by these just objects, to interfere with or endanger that ancient friendship between the two nations which had its origin in the struggles of the revolution. For such a service, no man in the United States was more eminently fitted than Mr. Livingston. The distinguished public office from which he had just retired, the ability and consistency which had marked his career as a statesman, his sound views in regard to the institutions and policy of his country, made him a representative of American feelings, opinions, and determination, in whom his fellow citizens had a perfect confidence. The known moderation of his character, his reputation as a jurist, especially on international questions, his long residence in Louisiana, whose inhabitants were connected with France by so many associations, his knowledge, which was more than commonly profound, of the language, literature, and history of that country, seemed to assure for him the most friendly reception there; and, as if to add to these circumstances of peculiar fitness for such a post, he had not long before been elected a member of the Institute of France: so that he was already enrolled among a body of distinguished Frenchmen, and connected with them by those ties which spring from mutual labours in the paths of science and of philanthropy, and in the search of wisdom and truth. He was accordingly selected, in the summer of 1833, by President Jackson, to fill the post of Minister Plenipotentiary to France. He accepted the appointment, embarked shortly after in the Delaware ship of the line, and arrived at Cherbourg in the month of September. He remained abroad until April, 1835, when he returned to the United States. Although, at the time of his leaving France, the differences between the two countries had not been finally adjusted, and his departure was a step taken in consequence of what he deemed due to the honour of his own country, yet it was shortly afterwards followed by an acquiescence on the part of the French government in the course which, under the instructions of President Jackson, he had firmly but temperately urged. His whole conduct, in circumstances which demanded at every step the exercise of an able judgment and an enlightened patriotism, served well to terminate his career as a public servant; and the official documents in which it is exhibited and vindicated, must ever be regarded as among the most excellent of his own state papers, and will deservedly hold a conspicuous place in the history of our intercourse with foreign nations.

Mr. Livingston did not long survive his return to his native country. He immediately resumed his residence at his estate on the Hudson river, among his numerous family connexions, and the rest of his life was spent in scenes rendered attractive to him at once by their own natural beauty, and by the associations of his earlier years. He devoted himself with the greatest enjoyment to the pursuits of the country. His farm and his garden, with that social intercourse in which he always loved to indulge, afforded him constant employment; and it was in the midst of such occupations that his life was terminated, by a sudden illness, in the spring of 1836. He had just reached the age of seventy-two.

The private life of Mr. Livingston was a daily exhibition of domestic and social qualities which secure affection and diffuse happiness; his temper was serene and his disposition cheerful; his heart was keenly alive to all the impulses of affection and of friendship;

he could bear misfortune with equanimity, but to the close of life readily participated in the cheerful amusements of society; devotedly fond of study, and having untiring industry and a retentive memory, his mind was richly stored with all the knowledge that literature could impart; fond of scientific investigations, so far as his many engagements permitted him to pursue them, he readily gave his aid to those who engaged in them; actively benevolent, he was unceasing in his endeavours to promote every plan which he deemed conducive to the welfare or improvement of men. In his profession he was eminently distinguished; as an advocate and a lawyer, he stood by general consent in the highest rank; and his labours in those kindred branches of study and reflection, which were required in the preparation of the systems of civil and criminal law which he framed, gave him a reputation, and secured to him honours and distinction in his own and other countries, not surpassed by any of the jurists of his times. Among the statesmen of America his place was no less eminent; his public speeches present in every instance striking views of the questions he discussed, and although the stations of trust to which he was elevated place his official labours in comparison with some of the most illustrious of his countrymen, this has only served to display more clearly their intrinsic merit, and to secure for them an equal approbation.

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## SPECIAL MEETING.

*Fourth Session, 27th May, half past 7 o'clock, P. M.*

Dr. Bache, Vice-President, in the Chair.

A letter was received from Dr. John Locke, of Cincinnati, Ohio, containing a brief notice of the method which he had adopted for replacing the cross hairs in the telescope of a transit instrument.

Dr. Locke remarks, that though this method may not be new to instrument makers, the description may be of service to amateurs who are at a distance from such aid. Dr. Locke used the threads taken from a spider's cocoon. The points to be accomplished were to stretch the line to a perfect tension in a manageable way, to re-